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VALUE IN HEALTH

Comments to the Editor on "Outcomes and Costs of Risperidone versus Olanzapine in Patients with Chronic Schizophrenia or Schizoaffective Disorders: A Markov Model"

To the Editor—We read with interest Vera-Llonch et al.'s modeling of their expected outcomes of schizophrenia treatment with risperidone or olanzapine. It seems to us that models are of greatest value when they lead us to new, unintuitive predictions about the relationships between variables. They may be especially useful when available data are not yet sufficient to answer questions of interest. Similarly, when actual data conflict with estimates from a theoretical model, the value of the model is suspect. Vera-Llonch and colleagues are modeling questions of compliance and cost that can be and have been measured directly. They fail to emphasize adequately that their conclusions are potentially misleading. The abstract highlights a striking conclusion that patients are more than twice as likely to discontinue olanzapine versus risperidone treatment. This is starkly contradicted by available data.

When evaluating medication compliance and its associated economic effects, we would direct the reader to go beyond hypothetical models and evaluate the actual findings. The authors acknowledge that randomized trials have not demonstrated the advantage in discontinuation rate that their model expects for risperidone. Furthermore, in naturalistic studies to this point, olanzapine use has been associated with greater adherence to medication therapy, and persistence on medication for longer periods of time relative to risperidone [1–7]. Additionally, at least four well-designed observational studies have shown that the total cost of treatment is actually equal or lower for olanzapine-treated patients versus risperidone recipients [2,5,8,9].

As results of the Vera-Llonch et al. model conflict with actual experience, the accuracy and adequacy of the model's assumptions are called into question. For example, although they assume that risperidone treatment is less likely to be associated with treatment-emergent diabetes than olanzapine treatment, we do not believe that available evidence [10] allows that conclusion. Furthermore, a large contributor to the model's results is its assumptions about proportions of significant weight gain between groups. The model assumes an incidence of only 3.7% for treatment-emergent weight gain (defined as an increase in body weight of at least 7%) with risperidone, which seems far below the 18% mentioned in the risperidone label for 6–8 weeks of treatment. Finally, discontinuation rates due to hyperprolactinemia and extrapyramidal symptoms were based on actual research data [11], 6% and 14%, respectively, whereas discontinuation rates due to diabetes and weight gain were arbitrarily set at a high level, 45% and 100%, respectively. It is likely that these or other dubious assumptions underlie conclusions that are contradicted by empiric findings.

The authors acknowledge that their model may be inconsistent with current clinical practice. Their manuscript would be more objective if it more clearly reflected throughout the potential fallacy of assumptions and conclusions.—Jerry D. Clewell, PharmD, MBA BCPS and Robert W. Baker, MD, Eli Lilly and Company, Indianapolis, IN, USA.

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